Gallo, Robert 2001

Dr. Robert Gallo Oral History 2001

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Interview with Dr. Robert Gallo (Clinical Associate at NIH in 1965) Cconducted by Sandeep Khot.

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What was the perception of other medical school professors or classmates toward the program?

It was extremely positive among those that were interested in biomedical research. I had come to the NIH when I was in medical school. I was awestruck by the place. It was like coming to Yankee stadium for a Yankees fan. It was truly an incredible place. So I knew when I finished medical school that I wanted to go back there. I don't think the war or the doctor draft were important at all in my decision to apply to the program. I just wanted the opportunity to conduct the type of research that was possible at the NIH.

Can you describe the research-training environment at the NIH when you arrived as a RA in 1965? Who were the leading figures in your field?

It was somewhat unique – not the only place to conduct that type of research but one of the better opportunities available. I think the NIH in many ways created its own competition by training many of the country's premier scientists who went on to medical schools across the country to establish biomedical research departments modeled off the Intramural Program. These programs began competing with the Intramural Program for the best research minds in the country. Now, unlike back then, there are hundreds of programs that offer what the Intramural Program does. So, in many ways, the NIH, through programs like the Clinical Associate Program, created it's own difficulties for the future of the Intramural Program.

Other associates have commented on how associates "taught each other" their respective field. Can you describe any special collaboration in which you participated as a Research Associate? Was it unique to the NIH?

I was extremely self-conscious at the time of my basic science training from medical school and so the first year I tried very hard to learn as much as I could about how to conduct biomedical research. So I had collaborations with many of the people I was assigned with on the leukemia wards. The following year when we were in the lab, they were studying the kinetics of leukemic cell turnover and I was in the same room with two or three other guys. We were cramped into a little room. In fact, the door would hit me in the head when someone came in. One of them, Ted Zimmerman, became one of the countries premier coagulation experts. He went to Scripps. But we used to interact with each other to each try to get over each other's insecurities. I knew a little more biochemistry than he did and he knew more cell kinetics. We were involved in both angles of it and we helped each other quite a bit.

Others have remarked on the flexibility of some of the Principal Investigators in allowing associates pursue their research interests in the way they wanted to. Can you elaborate on this?

Well yes, I was originally assigned to the lab chief of that department. He was also the associate scientific director. But he knew that I wanted more biochemical experience so he sent me to the floor below him, where I had some early chemistry training in enzymology and where I also saw my friend Harvey Marver to teach me some techniques on the side. That would be an example of plenty of flexibility. I also was scheduled to join Marshall Nirenberg' s laboratory the following year but then the Cancer Institute hired somebody from his lab, Sydney Peska, and I did a very formal training under Sydney Peska. Still our work was in the Medicine department which formed a new department called Tumor Cell Biology, which I eventually became the head of, and the head of that was the same man that headed the Medicine department. He gave me all of that flexibility for training that I saw fit. But I don't think that was anything different from any other place. It depends on the individuals. For example, when I was an intern at the University of Chicago, a man named Cliff Gorney, gave me his laboratory and access to reagents and a bit of technical help. And I published a paper all by myself in the Journal of Clinical Investigation that was a hundred percent in his lab. He was as generous as anyone that I ever met. So there was nothing in my mind that was special about the NIH in that regard. It was just special about individuals.

You have stated in a previous interview that when you came to the NIH in the mid-60's it was the best place in the world to be because "it was unique in that scientists were able to do research that could be applied to clinical medicine." What made it unique compared to the prominent research universities?

Well they weren't training people as much then. You remember how I said the NIH has created its own difficulties by helping form some of those centers. They didn't exist then. They were rare. There weren't cancer centers all over the country. They were only a couple. And there weren't neurology centers and centers for research for MD's to get involved in. You might find someone in a medical school and I did. I worked with a guy in a lab on erythropoeitin. But they were harder to find. There weren't many places for training of MD's to do research. At the NIH there was that. There may have been at a couple of medical schools that I didn't go to. I don't know. But I think that MD's all over the country were pointed toward the NIH. It was almost part of having to go there for a track record of success in academic medicine. Two-thirds or three-quarters of successful MD's running departments of medicine all went through the NIH. It was a great club to belong to and not just because it was the backroom boy's club but because of the training. It was a place that could bring basic research into the clinic rather uniquely. Not uniquely but rather uniquely meaning that it was not the only place on earth where you could do that but it did more of it with more opportunity and there were many places where you couldn't do that at all. So that and really the training and technology for MD's. There were so many things around you that you had somebody doing everything. The atmosphere then was unique.

Once you arrived, what about the ATP most appealed to you?

I don't know, it's hard to say. It was the whole milieu. The environment, the caliber of the associates, and the resources affected me a lot. The opportunity if you didn't know something, there was somebody there to pick it up from. A special place where MD's could be taken into a research world and all your ignorance, or many of your ignorances, and some of your stupidities could be taken away by the availability of courses, night and weekend courses where people gave up their time for that. It was special. The atmosphere of excitement, the substantial number of Nobel prize winners…how could it be more for somebody with my thoughts. It was a dreamland for me. And also what was special about the NIH was the opportunity that it provided to an unproven person. You could ask a question that you didn't have to answer within a year. There was funding for some years if you had a position. You didn't have someone breathing down your neck all the time. If they thought you were halfway good or a little special you were plenty of chances. I think that too is now passé. There are opportunities but it's more competitive now. I don't know why because there are less MD's interested in research but they seem to stay there.

Can you elaborate on how the style of your laboratory chief and clinical director during your training at the ATP influenced your style as a scientific instructor?

Yeah, I had training in both laboratory research and clinical at the same time. So I had always in my mind the strong desire to do basic biomedical research but with the twist that it could be in my lifetime clinically beneficial to someone. So I've always tried to keep a nose on the practical application of the research when possible. I think that was greatly influenced by my experience at the NIH. Being able to work in a group setting were there was technology that you would necessarily fear because there was always somebody around that could find it for you. So you could ask a question outside your expertise. The possibilities of broad collaboration added to the ability to ask very broad questions instead of only what you had your special technique for.

Do you think that most alumni from the program used the training they received at the time to train scientists in a novel way?

I have no idea how to answer that. I don't know what the others do.

Others have commented that much of the nostalgia for the ATP may be due to the fact that much of the intimacy when the NIH was smaller is gone and that the "open door" policy and knowing everyone on more of a first name basis is no longer as evident. Do you agree with this?

No I don't think so. You still have a first name basis among your world. The world is no larger or less personal than our world was. If something gets bigger you still end up with your collection around you. So I don't think that that's necessarily logically true. I think the reason for the nostalgia is that it was a period of time when by history and by the state of its uniqueness – it's no longer unique – it was a place that everyone wanted to go to that truly had an interest in research. So you had a lot of people truly interested in research A lot of people with the MD background who were among the best—we think we were the best—who came and influenced each other and were influenced by the senior researchers at the NIH. You tended to know them even if you didn' t know them because you heard their name because they became well-known. I never really knew Bert O'Malley well but I sure did learn of him and become social with him only after because he had already established a considerable reputation. The same for many others. It's endless in that period. So I think there is a pride and why not. You're among a select group that became the leaders in American biomedical science among MD's.

I think they're beating a dead horse when they try to see from the past how they can recapture…it's like a lot of things in that things don't stay the same. They're not in the same environment they once were. There's also another change. The politics changed. There was a time…I think of it this way. A congressman from Kentucky or Illinois or some other place didn't have his center so much to take care of but he had his pride at the NIH. He might have had something in his home state, of course, but soon every state had their centers, multiple centers and multiple places. Soon there was less pride in the NIH. And then science became much more political when things like…well like we have today in stem cell research or embryonic research before that. The ability to clone genes or the ability, as some people think, to play God. It drew more public attention and once you draw more public attention you draw more politicians, sometimes in good ways but often in not so good ways. The genome product…all these things make biomedical science much more visible and much more in people's face. Therefore, politicians know that there's a way to get attention a little bit. So you had the first taste of some of that whereas when I came in the mid-60's and throughout the 70's and into the 80's, we were the golden boys and we could do no wrong. You were just thought of as a hero. But then you have a period where people like Dingle came into science. When you have that kind of total lack of understanding of science or the scientific process and the extraordinary bullyism and arrogance to just send staff out to try and cause trouble in order to gain publicity with the sake that you're doing good for somebody but, in fact, you don't really care about getting the facts right. You just care about getting the visibility and potential power. That kind of thing was a disaster. It set back morale twenty years and set back science at the NIH for a number of years. It was not a small point. Very few people are going to tell you that for fear that that kind of thing can happen again but I don't have such fear of it. Because I went through it more personally than anyone else. So that would have been unthinkable in the 60's, 70's. As science becomes bigger, politicians become interested and in this way it was a negative. It was kind of like a Joe McCarthy business.

How did your experience in the ATP modify your career decisions?

Since basic science applied to clinical medicine in the best way we can is a real central theme of this institute or the very philosophy of this institute. Certainly the desire to find good young MD's and try to influence them to come into biomedical research and tell them all the fun it can be and the quality of life's experiences that can happen is probably a legacy of that period. The way you interact with younger people is obviously influenced by the way I experienced it there. I can't think of anything negative. Tremendous opportunities, good environment, good interrelations…it did a lot for American health.

Dr. Edward Rall has commented that the ATP has had a major influence on medical education because the addition of a serious research component to the training of M.D.'s who were going to end up in universities was pioneered here. Would you elaborate on that?

There's no question of that. That's why I came. I came to the NIH because

I thought I was going to be one of those things they called a "triple threat" – a super clinician, a teacher and a scientist. And then science got too complicated and so you really couldn't be all three. Nonetheless, I expected to come back to the university within a couple years but then I thought I didn't know enough science so I would stay a third year and really pour myself into this. And then the 4th year I thought I really like what I'm doing but I'll get back to medicine soon and I'll go to the university the year after. As a very young man, I had a terrific offer in Boston at the Children's Hospital with the late Sydney Farber. By then, without being able to say it to myself, I was hooked. The NIH was so exciting that though I knew that the thing to do was go back to the university but because of the thing what the NIH was doing, I got really hooked and I kept thinking I'll be young forever and it would always be there. But Rall is completely right and he epitomizes that angle because he is like an NIH fixture. I can't think of the NIH without thinking of a few names – him, Stetton, Earl Statmon…I had no training with Earl Statmon but he just seemed to be there all the time and somebody you knew his name before you got there, you knew his name when you were there and you just simply associate with the NIH so much. And we know each other probably just to say hello to. I don't know if he would recognize me. But those are the names that are the NIH. Of course, Shannon in my early years who was so famous. I never knew Shannon. And a few others…Don Frederickson, Wyngaarden. With Wyngaarden, I was already pretty mature. You remember the people when you haven't made it more. With Wyngaarden I was already pretty mature in my career. But we became friends and he's on my board here. I thought he was a great NIH director.

In your opinion, what has been the long-term effect of the ATP alumni on the academic world and scientific research?

Well, I think more than anything else I know of formed academic medicine in America today, which spilled over… as we learned from other countries and a lot of it being European based, I think we returned the favor. The early history here was based on European science and medicine and through largely, not solely but largely through the NIH, the policies of academic medicine and research were established in America. In turn, I think a lot of people in other countries have benefited from that model.

Dr. Fauci described how the work he did with the aberrant inflammatory response in Wegener's and the eventual cure came directly from the "benchwork" to "bedside" phenomena in the ATP. Can you describe any clinically driven paradigm shifts that came as a result of the training in the program, either in your research or others?

Almost everything in my career. I got interested in retroviruses because of my work in leukemia. And we found something that caused an unusual leukemia. I got interested in AIDS because I thought it might be a retroviral disease. And so those discoveries and the evidence of causation because we were interested in what was going on with AIDS. The discovery of interleukin-2 came because I needed to grow cells and went after factors that helped us grow cells. We discovered that chemokines are natural inhibitors of HIV because we were looking for natural inhibitors of HIV. I could go on and on. They were all directed from clinical observations. If I didn't know from clinicians that AIDS was a CD4 T-cell disease, I would never have worked on it because we knew how to grow CD4 T-cells and we knew how retroviruses particularly targeted T-cells. They're all based on clinical observations.

Did the collaboration with other alumni continue after you became a tenured intramural investigator?

Yes, with Sydney Peska. I would share contacts like with Sydney Peska. He is now chairman of microbiology at Rutgers in New Jersey. And we continued to periodically collaborate. There are other people also. Stewart Aaronson, the cancer center director at Mount Sinai. Him and I often collaborated. People who worked with me went out and sometimes I collaborated with them. Other times I would just call them up like when Caskey was at Merck, I would just call him up because I wanted to know if Merck was interested in business in some way. Look, Roy Vagilose, who I hardly know personally but we know each other and can pick up the phone and talk. Why? Because we know each other's name but also I also knew that he started at the NIH. So it's like that. There are scientists who I became friendly with through years of only periodic contacts. But it's because of. I'll give you another example of someone I was never close to while I was there. French Anderson I had a couple of collaborations with and some contact with. Warner Green, head of Gladstone, I'm collaborating with right now on the development of an HIV vaccine. He came as a research associate with Tom Waldman. We still collaborate with now.

*Could you discuss any **unintended** negative effect the program may have had in keeping minorities and women out of high level research positions as these groups were not represented in the ATP?*

I don't think that that is real in my mind because I can't imagine anybody in it or above it saying we're not going to have minorities or women. If you were to say they didn't pursue it actively enough, well I could tell you that women were not even going to my medical school when I was there. They started the year after I got out of medical school in the medical school I went to. So where were they going to come from? So this was not what was to blame. They just weren't there. And if you were to say did you bring in blacks, well from where? How many blacks were in medical school? And how many could afford to do research? I mean, there going to practice and make money if they can because in general they were not coming from wealth. People would have been prejudiced for women and minorities. If you saw them doing good research then why not? It's evident by who we took into our laboratories. My lab was full with women. I had two section heads that were women. Two out of three were women. If anything, you tend to get along with them easier especially if you're a boss. They're easier than guys a lot of times. For whatever the cause was the fact that it did happen, I believe, did hurt them in academic medicine for reasons we have gone through. This was a great advantage to have. I don't think it is true anymore or for the future because you don't have to go to the NIH anymore.

Dr. Sam Broder was quoted as saying, "My fear is that the intramural program does not function at that same level in terms of the interplay between the lab and bedside, and probably no place in the country now does. I think the NIH leadership clearly has not assigned full value to this function historically, in part because of the practical necessity, costs, and in part because of a lack of appreciation or respect for the process." Would you respond to this?

I don't agree with that. I agree with parts. I think that Sam is giving you a statement that overall there is not enough appreciation. True. But is this a policy that is set up by some NIH administrator? Well he was an NIH administrator. Did he set up the policy? I think he's really talking about individual policies but doesn't have the…he's not wanting to say specifics because it wouldn't be politically correct and might have repercussions for him. But there are individuals who can make that, it's an individual thing. But there's no policy at the NIH that you don't do that and most people do think it's terribly important and he did as a director of the National Cancer Institute. What he means is that has there been a strong NIH director to make that happen and he wants to say that some did not.

Some tried more in that way and some didn't try more in that way. My guess is that the next NIH director will try hard because my understanding is that the Bush administration is looking for someone whose more translational research and who has strong clinical capabilities and feelings. And I think the timing is right and there right on target to do that. I think the NIH needs it and in that sense I think he's right. But I don't see it as an organized plan. I see it as individual leadership only. But NIH is going to have problems. There are not enough MD's going into research. Everyone is going to have that problem. The whole crux of this is enough aggressive, intelligent, awestruck, young MD's who want to go into research and they are awed by it, privileged by it, not expecting things to be handed on their lap and their diapers changed for the rest of their lives. You know, which I see more of today, I think, among young MD's. My experience now is that the better ones are coming from Europe or Asia or the middle east or wherever else but not so much from America.

Do you think the sense of excitement, opportunity and determination that used to permeate the field is compromised by financial and career anxieties?

I suppose so. Whatever the reason is, it's not quite the same as before. Whether it's the financial or something else that's more subtle, I don't know.

In recent years, the concept of "translational research" has come to reveal a directional bias in which most basic discoveries are made in basic science labs and applied to the clinic. Can you comment on this? Do you think it has to do with the fact that the technology required to do biomedical research has become very sophisticated and it is difficult for many clinical research training programs to keep up with that sophistication?

Not in this institute. I don't know about everywhere else. Science is getting more complex and there's an awful lot that's on the plate to be translated where as before you struggled with it more. I can't say that I experienced what you're saying because this institute is geared toward translational research coming out of basic science. Going back and forth. Look, this is an institute that's built with a clinical division, an epidemiological division, an animal model division-- an animal model for what; for experimental medicine— a basic science division, and a vaccine division. I had the capacity to make this de novo in a way that I thought would be for the next period of science. That's the way I formed it. That surely was influenced by my background at NIH and for me this is the way I did things then and the way I do things now. It hasn't changed.

What kind of effect do you think there will be in not having a strong intramural program to handle public health issues like the AIDS epidemic in the 1980's?

Look the problem is that the strength is disseminated. You're looking for what was and what was will not be anymore in my view. "What was" was immensely valuable but whether it could go back to that, I believe you're beating a dead horse. That's my view. Things have disseminated across the nation now. You don't need to go to NIH anymore. There are centers all over the place. That's the difference in my view. I may be wrong. The second difference that you can't correct by having a great intramural program at NIH or the Associate Program is that young MD's are not coming into biomedical research as much as far as I can see. And can NIH correct that by a strong intramural program or if that program could come back? I don't know. I tend to doubt it because I don't see the same attitudes there.

Can you discuss the possibility today, in an atmosphere much more individualistic and less service-oriented, for the government to mobilize medical talent for specific objectives, let's say dealing with the AIDS crisis in Africa?

Yes, I could see that. It's an interesting thought.

Over the past few years, there has been a movement in our society to honor those who served in the Armed Forces during WWII and Vietnam. On the other hand, while the legacy of the ATP alumni has been enormous in altering American medicine as we know it, the recognition is lacking and there is still some negative connotation associated with the term "yellow beret". Are you are aware of any resentment or sensitivity among associates to this?

Not at all. We make jokes about the term yellow berets all the time. I guess if I came to NIH to avoid the draft, I would be super sensitive because there would be some truth to it but I know why I came to NIH. And I know why most people that I knew came to NIH but not all. But most came to NIH for the reasons I told you. I didn't come at the peak of the draft so it wasn't so intense for us anyway, for my era. Maybe that was a higher percentage and maybe there is some of that. But you're absolutely right that it's certainly not known. It would be nice that history got out properly but you can't make heroes out of it because we didn't give up our lives for anybody. We gained so you can't make us heroes. All you can say is that it was a wonderful and successful program that is an important part of American history – a very significant part of American medical history. And that is perhaps right in that if it wasn't for somebody like Victoria Harden and like you're doing it would largely be lost. And it's a big part of American history. That needs to be told but there's no heroes among the berets, whatever color they are. We didn't give our lives up, we gained.

In 1967, Rep. Daniel Flood of the House Appropriations Subcommittee on Labor, Health, and Education stated that "a ***quiet*** *revolution in the practice of medicine is taking place as a direct result of research." Can you comment on this and on anything else we should know about this program?*

It's now a noisy one. He was right then but I'm right now. It changed. Some of it's for better in that more people know about science but a lot of it's for the worse. They have politicized it more and there're more dangers. It's inevitable. When things get visible, you get good and bad with it. It cheapens it to some extent.

How do you think the ATP has changed, if at all, the reputation of the NIH?

I think it enormously made NIH famous among the MD's and everyone they're in contact with that care about biomedical research and training, clinical and laboratory. So everybody knew of that program. It's not true anymore. It's old but you'll help.

Dr. Gallo, thank you for allowing me the opportunity to conduct this interview.